

Reply to Referee #2: Interactive and Supplementary Comments

Impacts of a lengthening open water season on Alaskan coastal communities

Rolph *et al.* (2017), tc-2017-211

We would like to thank Referee #2 for her constructive comments, which have helped improve the quality of the paper. We present below our detailed responses to the comments in [green](#).

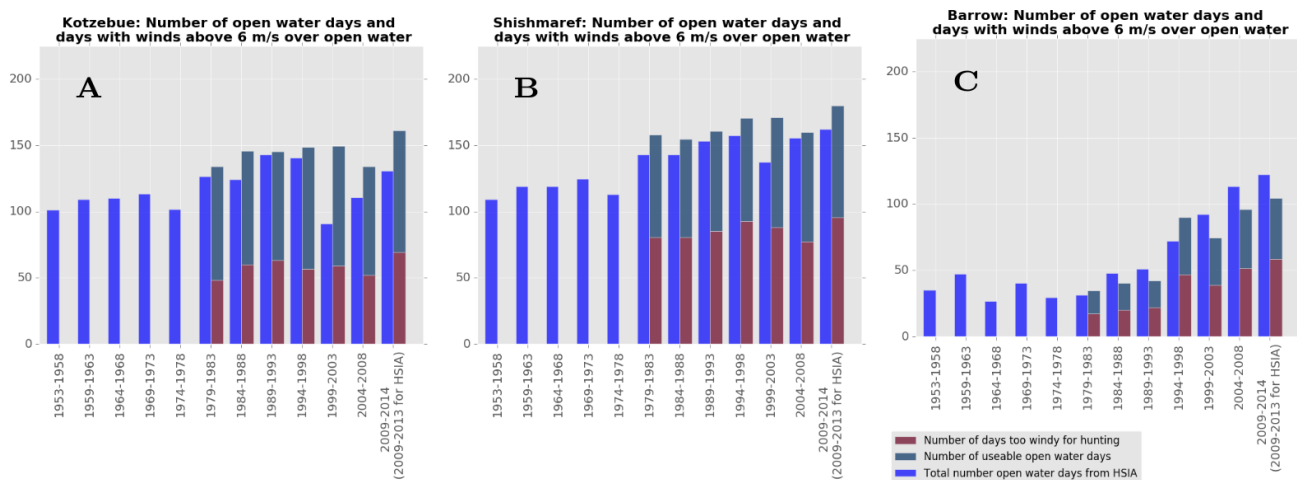
G. Ljubicic (Referee)
gitaljubicic@cunet.carleton.ca

Received and published: 30 November 2017

Interactive comments:

1. Does the paper address relevant scientific questions within the scope of TC? Yes.
2. Does the paper present novel concepts, ideas, tools, or data? Yes.
3. Are substantial conclusions reached? They need to be better supported.

Based on the reviewer's comments, we have restructured the paper to emphasize the results and discussion most directly relevant to the main conclusion: community-based metrics of sea ice have changed in recent decades. Please see our response to Reviewer #1, Question 3: We have provided a timeseries of the following indices for three communities from 1979-2014: Number of freeze-up/break-up cycles, number of open water days deemed too windy for offshore subsistence hunting, number of wind events capable of performing geomorphological work (erosion), or damage to infrastructure or habitats. From 1953-2013, we have also presented changes in the timing of freeze-up and break-up for the communities of Kotzebue and Shishmaref. In Utqiagvik, there has been an approximate tripling of the number of wind events capable of significant coastline erosion from 1979-2014. The sum of false freeze-ups and false break-ups have increased in recent years for all communities examined, especially Shishmaref. The number of days considered too windy for hunting via boat is increasing along with the increasing open water period for Utqiagvik, and maintains a significant fraction of the number of open water days in both Kotzebue and Shishmaref. These conclusions are community-relevant because we draw our thresholds from studies which have used indigenous knowledge to develop thresholds of climate related variables we were able to analyze from our large-scale datasets. We have also added the new dataset for wind, WRF-downscaled ERA Interim, as explained in our new manuscript. This dataset enabled us to extend the study to include the number of days considered too windy for hunting via boat (see sample plots below). The threshold for wind speed used is 6 m/s over open water, as reported by whalers to hinder whaling success due to high waves (Ashjian et al, 2010).



4. Are the scientific methods and assumptions valid and clearly outlined? See comments in attached .pdf.

Our responses to the reviewers comments here are given below in the responses to the supplementary material.

5. Are the results sufficient to support the interpretations and conclusions? See comments in attached .pdf.

Our responses to the reviewers comments here are given below in the responses to the supplementary material.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? In relation to HSI A analysis, yes. In others, need to be better supported.

We are assuming here based on the other comments that the methods of calculating the BSI index was not fully explained, and in the new manuscript, the BSI index has been removed. We decided to do this as well based on the concerns by the reviewer comments that it is not well connected to the rest of the paper. We also feel that the trends shown in the BSI index are captured, at least qualitatively, by the changes in the number of open water days at Utqiagvik, which is still included in the paper. The methods for calculating the number of open water days is given clearly in the Methods subsection "Selection of metrics to determine open water duration, freeze-up and break-up dates".

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes, but could be better referenced.

We appreciate the reviewer's comments here, and have added more references to the paper to support assertions made in the text and appropriately credit work of others. Some examples of the added references include work done by I. Krupnik (2002), Serreze et al (2016), and the references from which we have taken the climate-related thresholds for wind speeds over open water, Ashjian et al (2010), Solomon et al (1994), and Atkinson (2005).

8. Does the title clearly reflect the contents of the paper? Yes.

9. Does the abstract provide a concise and complete summary? Yes.

10. Is the overall presentation well structured and clear? Yes, but needs a bit more context in places.

In response to the reviewer's comments, we have restructured the presentation so that there is a consistent framework of subsections (for the different metrics) in the Methods, Results, and Discussion sections. We feel this has resulted in a more clearly organized manuscript.

11. Is the language fluent and precise? Yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? N/A

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See comments in attached .pdf.

Our responses to the reviewers comments here are given below in the responses to the supplementary material.

14. Are the number and quality of references appropriate? See comments in attached .pdf.

Our responses to the reviewers comments here are given below in the responses to the supplementary material.

15. Is the amount and quality of supplementary material appropriate? Yes

We appreciate the reviewer's detailed comments in the supplementary comments provided below, and have addressed those as follows (responses in green).

Supplementary comments:

November 30, 2017

The premise of this paper is to highlight the impacts of a lengthening open water season on Alaska coastal communities. Overall I find it an interesting and highly relevant paper. There is much current interest in defining impacts of environmental change according to indicators of relevance in a community context, and thus ultimately to help support decision-making at different scales. The extended record of the Historical Sea Ice Atlas (HSIA) is a valuable dataset for analyzing trends over time, although there are multiple challenges in doing so at appropriate scales and within particular community contexts. This paper tries to cover a lot of ground in a short paper. There are a number of good points made, although several areas where I would like more clarification, appropriate reference support, and nuanced discussion. It is also a paper well suited for discussion, and I look forward to reading feedback from other reviewers and discussants. Below are my contributions to this iterative review process, organized according to the key areas where I feel revisions would be needed prior to acceptance for full publication.

We particularly appreciate the reviewers overall comments here, especially the comment about there being interest in "defining impacts of environmental change according to indicators of relevance in a community context...". We have restructured the paper so that now multiple such indices are included and think this sharpens the main purpose of the paper: to demonstrate how utilization of large-scale datasets can be informed by indigenous knowledge in development of products that can inform stakeholders. In this case, the products of this paper are the long-term timeseries of our indices. The challenges of using the datasets at appropriate scales has been addressed here by adding the use of the references which have developed the thresholds informed by community stakeholders. An example of such a threshold is the wind speed threshold of lower than 6 m/s over open water for successful subsistence hunting and safe travel via boat (Ashjian et al (2010)).

1) Community uses of sea ice – Given the premise of the paper to investigate the direct and indirect impacts of a lengthening open water season on four Alaskan coastal communities (Barrow, Kotzebue, Shishmaref, and Nome), I think it would be important to have more characterization of geographic (e.g. physical conditions, typical sea ice extent and cycles) and cultural (e.g. uses of sea ice particular kinds of sea ice for particular hunting or harvesting practices, links to seasonal traditions or community events) context up front. It could be added to the introduction, or be in a new "community context" section, but without this it makes it hard to interpret some of the arguments being made later in the Discussion. The references to communities are highly generalized, without much sense of how their uses or priorities for sea ice may be shared or different, and this would help to strengthen arguments

as well as deepen the relevance of the analysis to the communities in question. Related to this, there is little explanation for the selection of the four communities beyond the diversity of sea ice regimes and subsistence activities (but these are not really introduced). In addition, Nome does not appear in most trend analyses, and Wales appears inconsistently in the text. More explanation and consistency in the communities of interest would be important. Furthermore, many of the references I am used to seeing that describe community use, conditions, and importance of sea ice from local perspectives in Alaska could be better incorporated throughout this paper to support both the local context as well as the analysis of direct and indirect impacts (e.g. work by Huntington, Eicken, Krupnik, Norton, George, Druckenmiller, among others).

We agree there was inconsistency in the extent at which different communities were examined in the previous paper. Based on the reviewer comments, we have decided to focus on the three communities: Kotzebue, Shishmaref, and Utqiagvik. Also based on the suggestion of the reviewer, we have added a subsection entitled “Characterization of communities examined” to the introduction providing characterization of these three communities. This new subsection provides context and sets up the Discussion section for the rest of the paper. It includes geographic position, typical sea ice cycles, and how the sea ice is used seasonally for traditional subsistence hunting practices. For example, a reference for the case of Utqiagvik is Gearheard et al (2006) “It’s not that simple”: a collaborative comparison of sea ice environments, their uses, observed changes, and adaptations in barrow, Alaska, USA, and Clyde River, Nunavut, Canada. *AMBIO: A Journal of the Human Environment*, 35(4), 203-211.

2) Selection of 30% ice concentration threshold – The choice of selecting 30% threshold for freeze-up and break-up needs more discussion and justification, as well as greater consideration of associated limitations. I find this to be quite low, if considering travel on landfast ice. It would also be very helpful to more clearly relate this to community use of sea ice. What would local perceptions of freeze-up approximate to in terms of ice concentration? You cited some of my previous work (Laidler et al., 2009) in which we used 9/10 (90%) ice concentration for freeze-up in terms of being navigable on snowmobile or foot (vs. 5/10 which is the common definition for freeze-up in relation ship navigation). At 30% concentration I would think there is still a lot of broken moving ice. Perhaps the overall trends would not change much, but this threshold selection is critical in terms of the arguments being made, and how this would translate to impacts on communities. This also has important implications for how transitional stages are considered, which are not really captured with one threshold (e.g. 30% used as break-up and as ice-free definition within the paper). I think this threshold selection and representation of transitional seasons is deserving of more careful consideration and/or articulation.

The purpose of our timeseries of freeze-up and break-up dates is to show the change or trend in freeze-up and break-up, which is, for the purposes of comparison across communities while using large-scale data sources, the date at which the sea ice concentration passes a particular threshold. Also in response to Reviewer #1, in their comments about section “Data and Methods”, we have also re-visited the explanation for choosing the 30% sea ice concentration threshold, citing Serreze et al (2016). While we agree that 90% is a more suitable threshold for over-ice transport, the 30% threshold is arguably a compromise between the more commonly used 15% threshold for “ice extent” and a threshold for on-ice travel. In addition, the dates for various thresholds are well correlated and so the observed trends are not particularly sensitive to the choice of threshold value.

3) Figure 1 – This figure does not give a good sense of scale of ice area covered around each community, or resolution of grid cells. Could a larger and more detailed figure be created to better represent this?

Because the ice coverage varies so widely over seasonal (and interannual) timescales, we believe that a depiction of the sample grid cells to illustrate the resolution provides the most “bang for the buck”. In the present version of Fig. 1, we show grid cells for the major communities included in the study.

4) Interview citations – Two interviews are cited in the text, and referenced as being interviewed in Kotzebue in 2013. There is no other context about these interviews in terms of how they were related to this research or other community-based projects, or any details in the Methods section about how interviews were undertaken and with what focus and which participants. I would like to see more of these local and Indigenous perspectives included in the paper, but they also need to be clearly explained and included in methods. Furthermore, interview quotes included from other papers need to be fully cited to the paper they were published in (as well as the individual), so they can be appropriately credited and contextualized.

We agree with the reviewer that this was not properly contextualized in the first version, and have made appropriate changes relating to both the citations as well as a clearer outline of the specific methods about how the indigenous knowledge was used in to obtain the main findings of the paper. The interviews now cited in the text are clearly credited with both the person being quoted and the source the quote appears in. The first interview comes from Ross Schaeffer, who was interviewed by Sarah Betcher in 2013. This is second interview quotation is by a hunter in St Lawrence Island, cited from work done by I. Krupnik (2002). The context for these interviews is more clearly given in the Discussion section. The quotation by Ross Schaeffer describes how the transition period seems longer in more recent years, and this is located in the revised Discussion subsection (4.1) which includes the changes in the number of false freeze-ups and false break-ups. The second interview is also located in this edited subsection, which describes how the increasing amount of open water can lead to a shorter period of break-up. In terms of the reviewers comment that she would like to see more local and Indigenous perspectives added in the paper, this is also included in the newly-added reference Ashjian et al (2010) because the authors of that study conducted interviews with local people in order to develop the wind speed thresholds we chose to use for our analysis.

5) BSI interpretations – This analysis does not seem well connected to the rest of the paper, and the calculations and methods involved are presented in the Discussion rather than Methods. Perhaps getting into this analysis in sufficient depth is beyond the scope of the paper? It would be good to really clarify what the primary goal and emphasis of the paper is. If it is indeed on community impacts (and related to community priorities and concerns), then expanding in areas noted above may be preferred to this particular aspect of analysis.

Based on this comment and the other reviewers comments, we have decided to remove the BSI analysis from the paper. Also see the comment by Reviewer #1 in the section pertaining to the Discussion.

6) Societal levels and accessibility arguments – I think what you are trying to refer to here is not societal levels (or scales), but decision-making or jurisdictional scales. This needs to be clarified throughout. The arguments here are also covered in such a generalized fashion, that it is difficult to connect to the sea ice and community-specific trend analysis. What would this mean in different community contexts? And when you talk about the accessibility of HSIA, to whom are you referring? How accessible and useable (and/or currently used) is the HSIA in Alaskan coastal communities?

We agree this section is not very well connected to the sea ice and community-specific trend analysis, especially in light of how the new manuscript has developed on the focus of the analysis of the indices

already based on thresholds from references using community-level input. Based on the extensive revisions to the updated manuscript, we feel this section did not really fit well with the flow for the rest of the paper, so it has been removed.

7) Typos and References – There are a number of minor typographic errors throughout, as well as a number of incomplete references, that need to be attended to. I can provide more details on these if requested.

We have revised the draft for typos, and checked for incomplete references. We would be interested to know which ones these are, in case something was missed that we are not aware of. Some examples of the more complete references include better citation of the interviews, as discussed in Comment #4 above.

In the process of trying to compile my feedback, a number of other questions have arisen for me. But I will leave it here to see what the other reviewers and discussants say, and how the authors choose to respond. I am then happy to continue being part of the iterative review and discussion process.

We sincerely appreciate the detailed feedback which we feel has significantly improved the revised manuscript and are open to additional feedback.

Best wishes,
Gita Ljubicic
Department of Geography and Environmental Studies
Carleton University, Ottawa, Canada